The Plastering of Wine.

The latest of the United States consular reports published by the State department contains a report by Walter T. Griffin, commercial agent, upon the plastering of wines. Since the great reduction in the amount of wines manufactured in the Bordeaux and Burgundy districts, the inferior wines of the central departments of France are being substituted for them, and recourse is had to chemical addition for the purpose of increasing their market-value. So important is this matter considered, that the question whether the plastering of wine is injurious to public health or not is now being discussed by the Academy of Medicine at Paris.

The plastering of wine consists in adding sulphate of lime after the first fermentation, or while the wine is in the vat; it is also mixed with the grape-must. The general rule is to put in five hundred grams of the plaster to the hectolitre of wine, but the greater number of wine-makers throw in the lime without weighing. The advantages said to be gained by the use of sulphate of lime are, that fermentation is greatly increased, is more rapid and complete, the color is brighter and more permanent, and the wine will keep for a much longer period. The objections are, that the addition of sulphate of lime causes chemical changes that render the wine injurious to health. The reasons given are these: wine, in its normal condition, contains a certain amount of bi-tartrate of potash, which, when brought in contact with sulphate of lime, forms an acid sulphate of potash, and there is precipitated an insoluble bi-tartrate of lime, varying according to its degree of alcohol, the wine dissolving a portion of the sulphate of lime.

Natural wine contains, at a maximum, about half a gram of sulphate of potash per litre. This quantity is increased from five to ten fold by the action of the lime, and at the same time the proportion of the bi-tartrate of potash diminishes to such a degree that it may be said that the lime substitutes for this salt the acid sulphate of potash. Finally, in wine treated with lime, sulphuric acid is found in a free state, also the sulphate of magnesia. There are three parties to the contest, — the proprietors and wine-merchants, who increase their profits by the plastering of the wine; the hygienists, who have always insisted upon the injurious effects of the practice; and the chemists, who have never given a final decision. The present discussion in the Academy of Medicine is the outgrowth of advice asked by the government of it and of the hygienic committees. A report of the progress thus far made in its inquiry by the academy has been made by M. Marty, who was designated to prepare it.

The paper is largely historical, and only a brief notice of that part of it will be made here. The hygienic committee, in 1856, reported in favor of plastering. The following year numerous evil consequences resulted from the plastered wines at St. Affrique, in the department of Aveyron. The doctors state that those who drank of this wine had an unquenchable thirst (cephalalgy) and an insupportable dryness of the throat. These are only the superficial symptoms and lesions that plastered wines produce in the organism. About the same time the Chamber of Commerce employed a committee of chemists to inquire into the matter, and they sustained the opinion given by the hygienic committee. In 1858 M. Poggiale, after new researches, found in the ashes of plastered wines an almost entire absence of bi-tartrate of potash, and an entirely abnormal proportion of sulphate of potash. He concluded that the practice of plastering had better be abandoned, as he considered it injurious to health. The *conseils generaux* entered into the lists after the decision given by the court at Roanne, which was against plastered wine. They demanded a new scientific inquiry. For a second time the hygienic committee, in spite of a spirited protestation from Michel Levy, declared in favor of plastering. M. Buignet and M. Bussy re-analyzed the plastered wine by a new process, and found free sulphuric acid, which was formed by the action of bi-tartrate of potash and sulphate of lime. The result was a compromise by the chemists, who considered that plastering might be done with moderation. In 1879 this question was brought for a third time before the committee, who did not admit the harmlessness of plastering, but said that two grams per litre were not dangerous.

M. Marty, in his report just published, settles the question from a hygienic view. He reports upon several experiments which have been made to show the harmlessness of wine plastered to 4°. All these experiments fail for want of precision or exactness in their method. It is an incontestable fact that plastered wines have occasioned functional troubles and organic injuries. All familiar with medical science know that a solution of acid sulphate of potash, in which sulphuric acid is in a free state, acts as a purgative, and a caustic in certain cases. In regard to the abolition of plastering, the hygienic committee are not unanimous in their decision. It is the opinion that a moderate plastering is necessary for the utilization, preservation, and transportation of a certain class of the poorer grades of wine, whose loss would be a disastrous thing for the wine-growers. But producers and merchants are warned, that, if they should continue the practice, the proportion of acid sulphate should not exceed two grams per litre. This proportion is sufficient to obtain the commercial advantages for which the lime is used. In conclusion, M. Marty examines and refutes certain arguments recently produced in favor of plastering. He recognizes the fact that the conditions of the non-combination of the neutral sulphate and the acid sulphate of potash are not well known, but says we have a law of nature that will guide the hygienists in the study of this question; viz., that natural wines never contain more than $\frac{65}{100}$ of a gram of the sulphate of potash per litre. The hygienists, on their side, do not ignore the fact that this is the maximum dose, and if it is surpassed it will certainly injure public health. In conclusion, the academy gave it as its unanimous opinion that plastering wine was a custom detrimental to health, and petitions that the law of 1880 be rigorously enforced.

MOTIONS OF THE SOLAR SYSTEM.[1]

NO other hypothesis has been suggested which offers such direct and complete answers to most of the questions which relate to the origin, structure, and unity of the universe, as Newton's law of gravity. It is but natural, therefore, that the majority of the problems which arise in regard to the motions of the solar system should have their origin in an effort to confirm that law.

The first attempt to apply Newton's law to all the motions of the solar system was made by Laplace. When, however, Lindenau and Bouvard undertook to compute their tables of the motions of the planets, a complete revision of Laplace's theory was found necessary. So enormous is the labor involved, that there exists, besides those mentioned, only one other complete set of theories and tables of the motions of the principal planets, — that of Leverrier. Leverrier's tables of the inner planets are now nearly thirty years old. His tables of the outer planets are much later, having employed his attention almost to the day of his death. His tables of Jupiter and Saturn were published in 1876, and those of Uranus and Neptune in the year following. Newcomb's tables of Neptune were published in 1865; those of Uranus, in 1874. Hill's theory of Jupiter and Saturn, which has for years occupied his attention, has at last been completed, and he is now engaged in preparing tables therefrom. These are intended to form a part of a complete series of tables of the principal planets now being prepared under the direction of Professor Newcomb at Washington. Another such series is also being prepared by Professor Gyldén at Stockholm.

The values of the co-efficients of the terms of short period in the motions of the principal planets are now pretty well known; and the same might be said of the secular variations, were it not for the difference between theory and observation which exists in regard to the motion of the perihelion of Mercury, which was discovered by Leverrier, and has been confirmed by Newcomb, in a discussion of the observations of the transits of Mercury, extending over a period of more than two centuries. The cause of this difference still remains unknown. The completion and comparison with observations of the new theory of the four inner planets, now being prepared under the direction of Professor Newcomb, will be awaited with interest, with the hope that it may throw new light on this interesting subject.

The only recent original tables of the moon's motions are those

[1] Abstract of an address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, at Cleveland, O., Aug. 15–22, 1888, by Ormond Stone, vice-president of the section.

of Hansen. These, like Leverrier's tables of the inner planets, are now more than thirty years old. These tables have been compared with observations, and agree fairly well with those made during the century preceding their publication, but not with those made before or since that time. The theoretical value of the acceleration of the moon's longitude is 6″; that found by Hansen from accounts of ancient total eclipses of the sun, 12″. Newcomb, however, considers these accounts as unreliable, and, limiting himself to the Ptolemaic eclipses of the *Almagest* and the Arabian eclipses of th *Table Hakémite*, obtains the value 8″.3, or, from the Arabian eclipses alone, 7″, — a value but little greater than the theoretical value. Dr. Ginzel, from an extended examination of accounts of ancient and mediæval total eclipses of the sun, concludes that Hansen's value requires a change of only a little over 1″. His solution, however, in reality depends upon the ancient eclipses alone. The only other theory of the moon comparable with Hansen's is that of Delaunay. This theory, however, is limited to a determination of the inequalities in the motion of the moon due to the action of the sun, on the hypothesis that the orbit of the earth is a pure ellipse, and differs from that of Hansen in that the inequalities determined are not expressed numerically, but only symbolically in terms of arbitrary constants.

While the co-efficients of the inequalities upon which Hansen's tables are based seem to be pretty well known, I am not aware that the tables themselves have been sufficiently checked, except by comparison with observations. Apparently the great desideratum now is a set of tables computed from Delaunay's theory in a completed form, or computed in some other way entirely independently of Hansen's. Until Hansen's tables are thus checked, it is questionable whether it can be safely said that the motion of the moon cannot be completely accounted for by the law of gravity.

The detection of the two satellites of Mars by Professor Hall may be considered the most interesting recent achievement in pure discovery. It was not till the discovery of these satellites that a means was offered for the accurate determination of the mass of that planet. No satellites of Venus and Mercury have as yet been detected, and the values at present assumed for the masses of those planets are very uncertain.

In 1788, just one hundred years ago, Laplace published his theory of Jupiter's satellites. This theory is still the basis of the tables now in use. Souillart's analytical theory of these satellites appeared in 1881. The numerical theory was completed only within the last year, and the tables therefrom remain still to be formed.

Bessel made a careful investigation of the orbit of Titan; but the general theory of the Saturnian system which he commenced, he did not live to finish. Our knowledge of the motions of Saturn's satellites, with the exception of Titan, was very meagre until the erection of the great equatorial at Washington. A difficulty in the determination of a correct theory of the motions of Saturn's satellites is the fact that there are a number of cases of approximate commensurability in the ratios of their mean motions. The most interesting case is that of Hyperion, whose mean motion is very nearly three-fourths that of Titan. In this case there is the additional difficulty that their distance from one another is only about one-seventh as great at conjunction as at opposition.

Our knowledge of the motions of the satellites of Uranus and Neptune depends almost entirely on the observations made at Washington. Quite accurate determinations of the masses of these two planets have been obtained. The large secular motion of the plane of Neptune's satellite, to which Marth has called attention, needs confirmation.

The number of the asteroids is so great that they have been the frequent subject of statistical investigation. The systematic grouping of the nodes and perihelia which exists was shown by Newcomb to be the effect of perturbation. Glauser finds that the grouping of the nodes on the ecliptic is a result of a nearly uniform distribution on the orbit of Jupiter. Professor Newton had previously found that the mean plane of the asteroid orbits lies nearer to the plane of Jupiter's orbit than to the orbit plane of any individual asteroid. Eighty-five per cent of the asteroids have mean motions greater than twice and less than three times that of Jupiter; and the mean motions of none approximate closely either of these, the two simplest ratios possible. The next simplest ratios lie beyond the limits of the zone; that is, there are no asteroids having mean motions nearly equal to or less than one and a half times that of Jupiter, and none nearly equal to or greater than four times that of Jupiter. The labor of determining the general perturbations and computing tables of an asteroid is as great as in the case of a major planet. It is no wonder, therefore, that tables have been prepared for scarce a dozen of these small bodies, and that these are already out of date.

Of well-known comets of short period, Encke's, which has the shortest period of any, possesses the greatest interest to the student of celestial motions, since it was from a discussion of the orbit of this comet that Encke detected evidence of the existence of a resisting medium which produces an acceleration in the comet's mean motion. This acceleration has been confirmed by the investigations of Von Asten and Backlund. The investigations of Oppolzer and Haerdtl indicate that there is an acceleration also in the mean motion of Winnecke's comet.

We have thus glanced briefly at the present condition of our knowledge of the motions of the principal bodies of the solar system. Only four cases have been found in which we cannot fully explain these motions, so far as known, by Newton's law of gravity. The unexplained discordances are the motion of the perihelion of Mercury, and the accelerations of the mean motions of the moon and the two periodic comets just named.

If we go beyond the solar system, we cannot tell whether Newton's law does or does not apply without modification to all parts of the universe. It is principally in the hope of answering this question that double-star observations are carried on; and, in the case of the many binary systems already detected, Newton's law is satisfied within the errors of observation. Nevertheless, this evidence is purely negative, and its value, it seems to me, not at all commensurate with the labor expended upon it, unless it be in the case of such objects as Sirius, whose observation may assist in the solution of the problem of irregular so-called proper motion. The angles subtended are in general so small that relatively large personal errors are unavoidable; so that, even though their motions be controlled by a law or laws of gravity widely different from that of Newton, it is not likely that such differences can be proved with any degree of certainty. It is rather to the study of the proper motions of the fixed stars and of the nebulæ, and then only after a lapse of hundreds and perhaps thousands of years, that we must look for a solution of this question.

SOME PHASES IN THE PROGRESS OF CHEMISTRY.[1]

SINCE the isolation of oxygen by Priestley, the search for new elements has been carried on vigorously, and the facilities for this pursuit have been much increased by the use of the delicate spectroscopic methods. The result has been to continually extend the list of bodies which are grouped under this head. The announcement of new discoveries during the last ten years has been especially large, over seventy bodies having been added to the list during this time. The largest number added by any observer has resulted from the joint labors of Krüss and Nilson on the absorption spectra of the rare earths, and reaches to over twenty. Should these discoveries be verified, the possible number of compounds which would result is something enormous, but, judging from experience, few are likely to survive a very searching inspection; yet one of them, 'germanium,' discovered by Winkler in 1886, has already been accepted as one of the missing elements in Mendelejeff's scheme, whose existence and properties he predicted.

Since the unit weight of hydrogen is taken as the standard for comparison, while the determination of the atomic weights of a large number of the elements has been made only through the intervention of oxygen, the ratio of the atomic weights of these two elements is the most important one to be determined, and many attempts have been made to solve this problem. The older experiments of Dumas and others were recently subjected to a careful scrutiny, and it was shown that they were not sufficiently exact. As

[1] Abstract of an address before the Section of Chemistry of the American Association for the Advancement of Science at Cleveland, O., Aug. 15–22, 1888, by C. E. Munroe, vice-president of the section.